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A NEGLECTED MEASURE OF FATIGUE

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Von Kries (1) seems to have been the first to have recognized the psychological implications of the maximum rate of repeated voluntary movements. His experiments were made by attaching an electric wire to the end of the fingers, which closed a contact when the finger tapped upon a metal plate. The experiments were not especially systematic, and maximum rate only appears to have been taken into account. We find, however, a distinct recognition of the neural character of the limit placed upon this maximum rate, and of its relationship to that of the incomplete tetanus in sustained muscular contractions, as also observed by von Kries, previously by Horsley and Schaefer (2), and by Schaefer, Canney and Tunstall (3). Two years later Griffiths (4) repeated and elaborated these experiments, with especial reference to tetanus of the loaded muscle. "There is a gradual increase in the number of muscular responses per second as the weight is increased up to a certain number; any increase in the stretching weight beyond this point is accompanied by a decrease in the number of muscular responses per second." His most important observation for us, however, was that this rate in tetanic contraction usually increases slowly until about the end of the first minute, and then decreases slowly. The fatigue phenomena, however, are very slight for the longest period observed, 2½ minutes. It is necessary to bear in mind this result for tetanic contraction for comparison with the much more marked fatigue phenomena obtained when, instead of sustained contractions we are observing repeated voluntary movements. The observed relationships between these rhythmic discharges in tonic contractions and the most rapid repeated voluntary movements, as well as clonic contractions and tremors, I hope to discuss subsequently in a separate paper. For the present we may confine ourselves to the most rapid repeated voluntary movements, which have since had considerable experimental development under the name of the "tapping test."

Although the first psychological observation is the work of von Kries, the later development of the test has gone on almost wholly outside of Germany. It is practically unknown to the

Psychologische Arbeiten, that home of generalized researches, especially with reference to continued work. In 1891 Dresslar (5) took up the matter independently, publishing the results of an extensive series of experiments upon himself, as well as minor observations on other subjects. A mechanical form of counter was used, the time being taken for 300 taps. The principle of the instrument, while giving no account of the individual taps, might register gross fluctuations in rate. The results, however, speak very poorly for its trustworthiness in this respect. It seems to have given no indication of the extent of fatigue or appreciation of its significance. Only the slightest evidence of "incipient" fatigue was noted, whereas later investigators have repeatedly found the fatigue losses to be very marked even within the first 100 or 200 taps. The subject worked at the limit of practice, and it is interesting to note that "it was thought that after practice this number (300) would not be fatiguing," whereas actual observation shows that fatigue loss may be even greater at the limit of practice than at its beginning. All sensations of fatigue, Dresslar notes, had ceased with the first few days of the work. This is in accord with the writer's observations; the sensations of fatigue disappear with continued practice, but the objective phase persists. The average rate at the limit of practice for 300 taps was 8.5 per second, which is abnormally high. The work was done with the right hand; a few experiments with the left hand gave an average rate of 5.3 per second. This places the right and left hands very much further apart than they appear in the studies of Bryan, Marsh, or the writer. It can hardly be taken as other than an indication that the practice of one member in this function does not essentially affect the performance of the other members; so far as I know it is the most important single datum that we have on this point. The rate was found to be decreased by physical, and rather increased by mental work; some unsystematic observations of the writer are in accord with this result. How much confidence can be placed in the daily rhythm records it is difficult to say, because a performance in the tapping test at the limit of practice exerts a "warming up" influence that extends over a considerable period. However, the writer's experience would entirely confirm Dresslar in saying that lack of practice would account for the failure of such rhythms to develop in his two other subjects. The analysis of the practice of these subjects, however, is almost certainly at fault, for practice improvement is by no means eliminated on the third day, and, as has been said, gives little or no immunity to fatigue losses.

Presumably because of its relative simplicity as well as its high statistical reliability, we next find the test pitched upon

as a measure of "voluntary motor ability" in two investigations of mainly educational bearing. The first of these is the well known research of Bryan (6). The apparatus here used is a form of mechanical counter, registering the number of taps made during a standard interval of time, uniformly five seconds. While the method can of course give only a very incomplete idea of the actual fatigue phenomena, it seems to have demonstrated them far more reliably than the instrument used by Dresslar. A lowering of the rate was observed after 10 or 15 seconds work; it actually begins somewhat sooner, but the method would hardly detect it. We have no precise data as to what happens after the first minute or less; Bryan seems to have found that this decrease goes on at a uniform rate for about 10 minutes, when it becomes slower but continues until a zero is reached after some hours. He notes the question of partial recovery from ordinary fatigue: in the writer's experiment it may be taken as absolute in less than three minutes of rest from thirty seconds tapping. The main body of the work, however, is concerned with the increase in gross rate as related to age,¹ and beside having nothing directly to do with fatigue phenomena, is too familiar to need recapitulation here; the main objection to it is that it hardly recognizes sufficiently the effects of either practice or warming up.

In the work of Gilbert (7) we have the first definite use of the test as a fatigue measure. The subject executed taps on a telegraph key for 45 seconds, the first and last five seconds being recorded. As a fatigue measure, Gilbert gives the per cent. of loss in rapidity during the last five as compared to the first five seconds. The susceptibility to fatigue as indicated in this measure decreases uniformly for both sexes with increase in age, the extremes for each sex being about 21% at six years and 14% at seventeen years. It is noteworthy that the boys show throughout a greater percentage of fatigue loss than the girls, though their initial superiority is sufficient to leave the balance still in their favor. Havelock Ellis has cited this result as an example of the "more continuous character of woman's activity," but there would seem to be a possibility that in the class of subjects used a motor measure of this sort would interest the boys more than the girls, and they would consequently try harder, and tire quicker.² In this connection it may be observed that the adult men used as subjects by the writer show on the whole about as much fatigue loss during

¹ See also, with reference to the speed of the different joints, Woodworth, *The Accuracy of Voluntary Movement*, Mon. Supp. 3, pp. 108-10.

² See, however, the results of Bolton, quoted below.

30 seconds as Gilbert's children did during 45, which may argue for inferior co-operation in the children as a group.

Gilbert (8) also reports a subsequent similar series of experiments, made upon school children in Iowa, the first having dealt with Connecticut children. A mechanical counter was used. The general results are similar, though there are some special differences of interest. As to the number of taps in 5 seconds, the Iowa boys begin at 22.1 at 6 and do 34.4 at 17, while the Connecticut boys do 21.0 and 35.0 at these ages. For the girls the figures are 22.3 and 33.8, 19.7 and 31.5 respectively. During the earlier years the Iowa girls are slightly superior, while the Connecticut girls were always inferior to the boys. An interesting aspect of the results is found in the classifying of the children by their teachers into 3 groups, bright, average, and dull. In gross rate, the bright subjects are about equal to the average subjects, the dull subjects somewhat inferior. As regards fatigue phenomena, the girls again uniformly lose less than the boys, though the difference is practically nil during the last five years, 15-19. As regards the differences in the rating of the children, the results are rather striking. The bright children lose more than either of the others at 6, and less than either of the others at 19; this progressive immunity to fatigue is rather less marked in the average, and least of all in the dull children.

The tapping test also figures among the experiments made by Gilbert and Patrick (9) upon three individuals every 6 hours during a 90 hour sleep-fast. The graphic method was used, the subject tapping for 60 seconds; only the first and last five seconds, however, were counted. No account is taken of practice, which probably obscures most the results somewhat. The rates show little progressive change, but vary more than ought to be expected. The per cent. of loss by fatigue is irregular in the second subject, rather decreases in the first, and increases in the third. It should be noted that the scores of the tapping performances of the first subject are more regular than those of the other two and they may be more trustworthy, since he had probably had more practice with the test. The time of isolated individual taps has also been measured by Seashore (10), and compared with simple reaction time to various types of stimulation.

Contemporaneously with the first work of Gilbert, and also in the same laboratory, Bliss (11) introduced a great improvement in the method, though it involves complications which would render it unavailable save in laboratory practice. Bliss directed attention mainly to the variability of the individual taps, which he seems to be the first to have studied. The method was, of course, graphic, the time intervals being regis-

tered by a magnet in circuit with a 100 v. d. fork. The taps were recorded by the passing of a high tension spark from the tip of the recording point to the drum. It is unfortunate that with such an improved method available more experiments were not performed; the records, each of 180 taps, are only six in number, and all but one are on the same individual. The averages of the records show unmistakable fatigue losses, though I am inclined to think the fluctuations in rate are greater than would ordinarily be found; *i. e.*, the decrease in rate would be more regular. Bliss also speaks of a decided warming up in the rate of the individual taps during the first second; this also appeared in the subsequent experiments of Moore, and while the method used by the writer does not lend itself to very accurate determinations on this point, this initial warming up appears also to be generally present in his experiments. Fatigue losses begin roughly after about five seconds. His essential point, that the variability of the taps decreases as fatigue establishes itself, the results seem to indicate with some assurance.

Moore (12) subsequently used the same method of recording as Bliss, but so modified the method of making the taps that his results are not strictly comparable with those of the other investigators. The movement was confined to the index finger which shifted a slide 5 mm. forward and back as rapidly as possible. So far as the initial warming up in rate is concerned they agree with Bliss, but fatigue, which again appears remarkably late, seems to increase the variability of the taps. The gross rate is relatively low. These series were each about 480 taps long, which the investigator considers very near the point of complete exhaustion. When it is remembered that the freehand tap can be continued at maximum rate for an hour or more, it is quite apparent that the fatigue phenomena of these results are in no way comparable to those obtained by the previous investigators.

In studying practice-transference in this function, Davis (13) reverts to the tap upon a telegraph key, recording the number of taps per five seconds upon a mechanical counter. The results are very peculiar. The gross rates are exceedingly slow; the slowest individual among some thirty normal and pathological subjects tested by the writer would come at almost the average of Davis's subjects. The right toe was the member practiced, as it was endeavored to determine the influence of this practice upon the hands and left foot. The member practiced did not always improve, nor when it did was its improvement always greater than that of the unpracticed members. Upon one subject is recorded an experiment of 900 taps without fatigue loss. Altogether the findings are so out of key with practically the entire remaining literature of the method that it is very difficult to judge of their relation to it.

Binet and Courtier (14), and, more recently, Raif (15), have called attention to the relation which the tapping test bears to facility in playing the piano. The former investigators used the graphic method with air transmissions, giving some idea of the force of the taps. Rates of 6 to 10 per second were obtained for single fingers, the difference between those practiced and unpracticed on the piano being not so much in the gross rate as in the regularity of the force employed. Raif also finds the educated steadier, rather than faster, than the novices, and seems to make an *a priori* assumption that the educated are faster than the ignorant.¹

Binet and Vaschide (16) introduced a considerable modification, which might at first sight seem to be also a considerable improvement, into the method. They object to the telegraph or other key as affording too incomplete an analysis of the movements involved. Instead, a Mosso ergograph was modified into a sort of myograph, using a weight of 1 kg. to be lifted and released as rapidly as possible. This isolates the movement much as in Moore's experiment, and also greatly increases the muscular effort. But as von Kries has pointed out, the significance of the experiment is essentially dependent upon having as free a movement as possible, a condition which his own experiments, perhaps, realize as completely as any have done. Moreover, it must be noted that if we isolate small muscles like those of the finger, and especially if we weight them, we complicate the experiment with a second fatigue factor whose relation to the specific fatigue phenomena of the test it is very difficult to estimate. We fatigue the muscle with reference to the force of its movements as well as to their speed. Various considerations render it desirable that whatever joints we are testing for this function should have as nearly constant muscular power behind them as possible. These conditions are probably best obtained by allowing the subject to select his own preferred method of tapping, and simply to see that he maintains it throughout the experiment. Under ordinary conditions this usually amounts to a combined wrist and elbow tap, with the elbow rested upon the table. No one will dispute that under proper conditions the myograph is ultimately more precise and accurate than the key, but such an instrument would have to be of so delicate a construction as to be very inconvenient for ordinary experimental work, while its advantages as a measure of individual differences in the function would be largely factitious, as Binet and Vaschide themselves admit.

In spite of these essential differences in the isolation and

¹ Cf. also Davis's paper, p. 13.

loading of the muscles, their results are not very different from what one would expect from the previous researches. The tests were twenty-five seconds in length in 15 subjects, and the rates varied from 3 to 8 movements per second; from considerably below to rather above what is ordinarily obtained. The average rate at the beginning of the experiment is given as 5 per second, that at the end as 3.5 per second. There is thus a considerable fatigue loss in rate which the investigators state to be regular, though no precise measurements are indicated. Such analyses as are given indicate that the decrease is not regular, but, as natural, most marked at the beginning. The amount of loss is rather more than one would expect, owing presumably to the loading of the muscle, and shows more marked individual differences than are usually found; it is a question whether these do not as much represent differences in co-operation as in ability for the test itself.

A return to the simplicity of von Kries's procedure is found in the method used by Marsh (17). The subject tapped upon a metal plate with a stylus held in the hand, the taps being recorded by an electric counter similar in principle to the Ewald chronoscope used by Gilbert. The unit of the experiment was the time taken to make a specific number, usually 100 taps, as measured with the stop watch. A large number of experiments upon different groups of subjects gave average rates varying for the right hand between 6.7 and 7.5 taps per second, and for the left hand between 5.8 and 7.2 taps per second. These ranges are small because the averages do not represent records of individuals but of groups; the relation between the right and left hand is about the same as that obtained by the writer. It will be remembered that the afternoon records quite generally surpassed those taken in the morning. This investigator also performed an extensive series of experiments on himself, taking the time for 200 taps at different portions of the day. These results are in conformity with the above, but they do not maintain a very close correspondence with the daily rhythm curve of Dresslar; moreover, the later periods of the evening, when Dresslar did not work, are found to be the most rapid of all. No account is taken of fatigue phenomena, but so far as the gross rates are concerned, these experiments probably combine extensiveness and reliability in as good a ratio as any that we have.

There are three more or less general researches of educational bearing that have also made use of the test, among others. Bagley (18) employed a telegraph key and an electric counter, finding no special correlation between tapping rate and class standing. Bolton (19) employed a mechanical counter, in a series of experiments with much the same object, obtaining

rather contrary results. Two groups of children were selected, apparently mainly according to home environment, and submitted to various tests. The groups consisted of children of eight and nine years. Between the right and left hands no significant difference between good and poor was found, but in respect to gross rates, the good at nine differed more from the poor at nine than the good at eight did from the poor at eight. Also did the good at nine differ more from the good at eight than the poor at nine did from poor at eight. The statistics are not altogether satisfactory, but the differences would seem large enough to be significant, and to have the interpretation which Bolton suggests for them. It is also a very suggestive result that in successive trials the good children improved, while the poorer lost slightly; but the phenomenon is one to which I should prefer to apply the term "warming up" rather than practice, which I would reserve for gains of a more permanent character than these seem to be. As regards sex, the girls tap faster than the boys, in direct opposition to the first results of Gilbert. If this difference is significant it is also suggestive that the poor girls are more superior to the poor boys than the good girls are to the good boys.

Kelly (20) improved the evaluation of the test by having the subject tap for 60 seconds, taking the reading every ten seconds. A fairly precise fatigue curve can thus be obtained, but unfortunately the readings cannot be made accurately enough, at least with a subject who taps at all rapidly. This is at present the most objectionable feature of accurate work with the tapping test; the graphic method is necessary, and its evaluation very tedious. Nevertheless there can be but little doubt that should the test demonstrate sufficient special value as a fatigue measure, these difficulties could be obviated through special apparatus.

An interesting observation is reported by W. G. Smith (21), who used the graphic method and air transmission in comparing the rates of normal and epileptic individuals. There is probably no significant difference in the gross rates, whose average is about normal, but while the normal average was 6.3 per second for the first eight seconds and 5.9 for the second eight, in the epileptics it was 6.2 for the first eight and 6.3 for the second eight. The normal individuals fatigue, while at least some of the epileptics must have warmed up considerably during this period. This phenomenon is very rare in normal individuals, but has been observed with some regularity in certain psychoses; its interpretation is by no means clear.

To sum up, the maximum rate of repeated voluntary movements is a function that practically every investigator working with sufficiently accurate methods has found to be subject to

fatigue effects, though the degree of this subjection has differed considerably. Nothing is definitely known regarding the relation of fatigability to the gross rates; *i. e.*, whether faster individuals are likely to fatigue more than slower, nor is the significance of the gross rate itself well understood. Little if anything can be regarded as established in regard to the practice phenomena of the function either in respect to gross rate or susceptibility to fatigue. We have a rough knowledge of the limits within which the initial rates vary, *i. e.*, from about 5 to 14 per second according to the individual; it has not been brought out in what way this rate is correlated with other and deeper mental faculties in the individual, except in so far as is given in Gilbert's and in Bolton's figures. In spite of these considerations, the writer's experience with the test, amounting to some 1,500 individual fatigue curves upon upwards of 30 subjects under many varieties of conditions, seems to justify the belief that we have here an experiment that will in every way bear comparison with such fatigue tests as have found more general employment as measures of this function.

When we speak of a measure of fatigue we may mean either a measure of the state of fatigue or of the susceptibility to fatigue.¹ In measuring experimentally a state of fatigue we usually have certain objectively given or assumed fatigue conditions, and we attempt to determine what has been the effect of these conditions upon some function or functions subject to psychological measurement. Various forms of psychological tests have been used in this way. Griesbach made the suggestion, now rather gone by the board, of cutaneous sensibility; the *Kombinationsmethode* of Ebbinghaus is a form of the uncompleted word test; Ritter has proposed a form of our own familiar *A* test. There is little limit, other than the purely mechanical, to the experiments that can be applied in this way. But as has more than once been pointed out, there are great difficulties in the way of using the mere optimum performance in a given test as a measure of the state of fatigue. This optimum performance, especially in school work, is affected by entirely too many other conditions than those it is here used to measure. Even such elementary things as interest, distraction, and rivalry can have a considerable influence upon the gross performance, and are quite likely to vary independently of the fatigue process itself. The reader may remember the interesting observations of Schuyten (22), who experimented with auditory memory, testing the children morning and afternoon of the same day, and afternoon of the first day and

¹ Upon some of these points the writer has already touched, from a slightly different angle, in an article in the *American Journal of Insanity*, LXIV, pp. 502 ff.

morning of the next. Whether made in the morning or afternoon, the children did better in the first test than in the second. The only inference that is in any way justified by such an observation is that the factor of novelty was more potent than that of practice; but in motor accuracy the reverse condition might well have obtained.

Such experiments as these belong to the following type; an initial test a , is followed by a period of supposedly fatiguing work w , after which is made a second similar test a^1 . The entire result is given in the difference between these two measures, a and a^1 , and is absolutely dependent on their validity. The susceptibility to whatever condition is brought about by the work is given in the relation of the two quantities, but a^1 is not necessarily less than a ; indeed, there are numerous instances from the literature in which the second test shows a marked gain over the first. And we must remember that the same relation between a and a^1 may be brought about by very different fatigue conditions. The measures a and a^1 have often been so unprecise, and the work done between them so ill-controlled as to give really no reliable criterion of susceptibility in the individual, and only a very slight one for the group.

But perhaps the greatest objection to be brought against this type of experiment is that it gives so little opportunity to distinguish between the individual who is already so fatigued as to suffer little fatigue loss during the experiment, and the individual who, though unfatigued, suffers as little through a relative immunity to fatigue. The only clue that it gives at all is in the gross scores, and, as has been said, the individual measures are so coarse as to make this criterion quite unreliable. The situation is complicated by the fact that the former class is likely to be made up largely of individuals highly susceptible to fatigue. But when we measure a fatigue in terms of itself, *i. e.*, study the actual fatigue phenomena of a certain function, there is reason to believe that we can differentiate these groups through factors largely independent of the gross scores.

And in this, I think theoretically justifiable, shifting of the viewpoint from the measurement of discrete states of fatigue to continuous determinations of susceptibility, the problem is otherwise considerably simplified. We largely eliminate the errors arising from the differences in attitude toward separate tests, because the measure consists of a single test. Of course, if the single test is a prolonged one, such errors are more likely to introduce themselves, but from the very fact of its being more extensive, they are less likely to lead to a false interpretation of the results. We are no longer attempting to measure the fatigue due to the continuous exercise of one function by its effect on discrete performances in another, a procedure

whose logic often suggests measuring the width of the mouth of the Amazon River by taking soundings in Chesapeake Bay; but we are using continuously a single function of determined efficiency of performance throughout. There would seem to be little room for comparison between the validity of the two procedures, always bearing in mind the question, is there such a thing as general susceptibility, and if so, to what extent may individual measures be expected to reflect it? In making such selections we are again in danger, to reverse Dr. Edes's expression, of inferring a rise of the Mississippi at the mouth from the occurrence of a thunderstorm somewhere in Minnesota. We are not yet, and perhaps never can be, fundamentally sure of how far it is justifiable to judge of a general fatigability through a determination restricted to a few narrow functions.¹ The most we can do is to select from the measures available for the study of fluctuations in continued work, such as shall best obviate the sources of error peculiar to this class of tests, as well as best meeting the more general requirements of psychological experiment.

There seems to be but little present reason for giving *a priori* preference to any special type of fatigue measures. Of course, if we were attempting to measure the fatigue of some special function we should naturally cast our test as nearly as possible into terms of that function; but using the term in a more general, or, if the word may be permitted, a more abstract sense, there is slight, if any, theoretical reason for preferring a motor test over a sensory, an intellectual over a motor. We must consider them upon their merits, as psychological tests. There is probably no test that entirely obviates any of the sources of error mentioned; but they are certainly subject to them in widely varying degrees, and differ widely in the precision with which they reflect ability in their special fields.

First of all, such a measure should make as few demands as possible on the conscious co-operativeness of the subject, because this introduces a large additional variable, absolutely uncontrollable, and of very ill-understood significance. The degree of co-operation accorded should be as constant as possible, and this end is probably best secured by making the degree demanded as small as possible. Kraepelin's addition test, which has figured so largely in the fatigue literature of the higher mental processes, has two considerable defects, and this is one of them. Each successive addition in Kraepelin's test requires conscious readjustments of no little complexity, and their in-

¹ We must also consider to what extent one's fatigability at high, experimental pressures of work may be correlated to that at ordinary, moderate pressures of work.

creasing irksomeness has been held in no small degree responsible for such fatigue phenomena as the workers with this test have observed. No doubt these readjustments tend to become automatic after a certain degree of practice is reached; but in ordinary fatigue experimentation we deal with individuals at practically the beginning of practice in such a test as this. In the earlier stages of practice consciousness cannot wander a hair's breadth from the work in hand without seriously affecting its amount, and these wanderings are equally difficult to prevent or control. In this respect it yields to the ergograph, though this instrument makes the frank assumption that "maximum" has the same meaning for consciousness at each pull. Moreover, the curves are considerably complicated with fatigue sensations; the weight instruments probably more so than the spring.

The feelings of annoyance arising from a long continued test make it desirable that the experiment should be one giving the requisite data in as short a time as possible. Here the Mosso instrument stands out best, and in the spring ergographs the characteristic phenomena also appear far sooner than in the tests of the higher mental processes, if indeed, as ordinarily observed, these tests show any objective fatigue phenomena at all. In this connection it is very interesting to note the method suggested by Squire (23), which consisted of the indefinite repetition of a rather complex motor act, recorded upon a kymograph. While the test was thus motor in character, the measure of fatigue was concerned with the higher mental processes, being given in an increase in the lapses and irregularities in the performance of the act. Considered from the utilitarian standpoint, the experiment requires a considerable time to make, and demands a degree of co-operation that would probably render it unavailable save among subjects of special training. A quantitative statement of the findings of such an experiment would be exceedingly difficult, in fact, none is attempted; and little practical value can be attached to a test that does not readily lend itself to this treatment. The study is most worthy of note as an attempt to analyze out experimentally a certain source of error, as that of muscular fatigue with the ergograph, and so to limit objectively the interpretation to be put upon the fatigue phenomena observed.

Thirdly, such a measure should be precise. This is the other weak point of the addition test. As at present given, we can measure the total amount of work done in a fixed interval, say 15, 30, or 60 seconds, or we can measure the time required to perform a certain number of additions, say 50. What goes on within these periods is absolutely hidden from us. We have no objective means of knowing whether a period of decreased

efficiency is the result of a gradual slowing down of the associative process, or through one or more extensive wanderings of the attention. Such elementary considerations cannot be arrived at unless the individual processes are recorded, and in the addition test this is impossible to do with any accuracy. Moreover, there remain the errors. As there is no way to deal with them, they are usually disregarded; but they are far from being shown to be negligible for the significance of the test.

Another factor must be mentioned, which is of vital importance where there is any question about the co-operation, as in clinical work. Unless the answers in the addition test are either spoken or written, there is absolutely no objective evidence as to whether the work has been done or not, save in so far as one might infer it from the figures of the results obtained. From these objections it will be seen that the ergograph is relatively free. The objective record of the work is there, and is not complicated with two such incommensurable factors as amount and error in the addition test. In precision, also, the ergograph record leaves little to be desired, though it must be remembered that this precision is not necessarily synonymous with accuracy, at least in the majority of the instruments. Nevertheless, so far as concerns the technical points of speed, objectivity and precision, there can be small doubt of a consistent superiority of the motor measures over the intellectual. Only in the simplicity of their apparatus have these latter an advantage, and this advantage is considerably discounted by the fact that it involves no corresponding simplicity of procedure for the subject.

It is worthy of note that so much attention has been given to the force of movement, to the relative exclusion of its other functions. It is difficult to imagine any *a priori* reason why the force of movement should have any special superiority as a psychological measure. Our primary aim is a measure of a neural process, and every one knows the extent of the discussion as to whether the ergograph is really a measure of nervous facts at all. On the contrary, we find in the maximum rate at which voluntary motor innervations succeed each other, a fatigue phenomenon so entirely foreign to what is observed unless nervous elements of some degree of organization are concerned, that there seems to be little, if any, escape from the conclusion that this phenomenon is at least mainly of nervous origin. Further, it is probable that on the purely technical side its experimental efficiency is at least equal to that of the ergograph. It is comparatively certain that it demands less co-operative effort; records of a considerable degree of trustworthiness have been obtained from depressed and demented subjects who would have been quite unamenable to ergographic

experiment. Sensations of fatigue at any stage of practice seem to influence the results to a minimum that is not approached in any other fatigue experiment with which the writer is familiar. Neither gross efficiency nor susceptibility to fatigue seem correlated to any significant degree with the subjective estimate of speed or with the sensations of fatigue that accompany the work. In the time required by the actual experiment, the test also makes a favorable showing; quite sufficiently characteristic fatigue phenomena normally appear during a period of 30 seconds, which interval has been selected as the standard in the writer's experiments. In using the simple tap, its ultimate precision is probably inferior to that of the ergograph, though it is quite equal to it so far as the actually evaluated factor, *i. e.*, the number of taps is concerned. Moreover, this objection could be entirely overcome by using a myographic instrument, as did Binet and Vaschide. There remains the tediousness of evaluation, in the counting of the individual taps; but it is probably better that the evaluation should be tedious than that it should be relatively unintelligible, as in the ergograph, or relatively meaningless, as in the addition test.

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